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BENACERRAF'S DILEMMA AND INFORMAL MATHEMATICS

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Abstract. This paper puts forward and defends an account of mathematical truth, and in particular an account of the truth of mathematical axioms. The proposal attempts to be completely nonrevisionist. In this connection, it seeks to satisfy simultaneously both horns of Benacerraf's dilemma. The account builds upon Georg Kreisel's work on informal rigour. Kreisel defends the view that axioms are arrived at by a rigorous examination of our informal notions, as opposed to being stipulated or arrived at by trial and error. This view is then supplemented by a Fregean account of the objectivity and our knowledge of abstract objects. It is then argued that the resulting view faces no insurmountable metaphysical or epistemic obstacles.

§1. Introduction. Benacerraf's (1973) paper 'Mathematical truth' presents a problem with which any position in the philosophy of mathematics must come to terms. Benacerraf's paper is often seen as presenting a *dilemma* where common sense seems to pull in opposite directions. Common sense with respect to the truth and the syntactical form of mathematical statements leads us to conclude that mathematical propositions concern abstract objects. At the same time, common sense with respect to epistemology seems to imply that mathematical propositions cannot concern abstract objects. It is quite generally accepted that no existing position on the philosophy of mathematics is completely adequate in its handling of the Benacerraf dilemma. It is the goal of this paper to show that a position on the philosophy of mathematics that will strike most practicing mathematicians as very natural can aptly handle the Benacerraf challenge. The position I outline is inspired by the view that Kreisel (1967) presents and defends in his *Informal rigour and completeness proofs*. However, the position I put forward here is in certain respects different from Kreisel's own.[1] The principal difference is the supplementation of the Kreiselian account with a Fregean position on abstract objects and the epistemology thereof. I will outline two very natural constraints on an account of mathematical truth. I will then show that these constraints are not only natural but also can be accepted without causing any insurmountable philosophical problems. I aim ultimately to show that a Kreiselian position on how we arrive at axioms together with a Fregean position on our knowledge of abstract objects gives us an extremely natural solution to Benacerraf's dilemma.

Section 2 of this paper will outline the Benacerraf problem. Since Field's reformulation of the Benacerraf problem (so as not to presuppose a causal theory of knowledge and

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[1] In fact the final position will be somewhat Carnapian. Since, to discuss in detail in what respects the position is Carnapian would take us too far off course, I will simply refer the reader to my Lavers (2008) for a detailed discussion of the relation between Kreisel and Carnap's philosophy of mathematics. Mention of Carnap will therefore be limited.

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reference) is the version on which most contemporary discussion focuses, this reformulation will be presented as well. Section 3 will be a brief presentation of Kreisel's views on informal mathematics. In Section 4, I discuss Field and Kreisel on the epistemology of mathematics. I conclude this section by presenting a principle linking mathematical truth with what we can demonstrate, and I argue that it should be accepted barring any insurmountable metaphysical or epistemic reasons not to. In the fifth section I discuss mathematical ontology. I introduce another principle guaranteeing the disquotation of mathematical claims. I further argue that a Fregean view on abstract objects can be used to show the two principles just mentioned can be accepted without the introduction of insurmountable philosophical problems. In the final section I consider several objections to the account that I put forward.

§2. The Benacerraf dilemma. Benacerraf (1973) begins his paper by proposing two concerns for a theory of mathematical truth:

(1) The concern for having a homogeneous semantical theory in which the semantics for the propositions of mathematics parallel the semantics for the rest of the language.

(2) The concern that an account of mathematical truth mesh with a reasonable epistemology. (p. 73)

The reason for the second condition is obvious, but why should we accept the first concern? Benacerraf asks the reader to consider the following two sentences:

(1) There are at least three large cities older than New York.

(2) There are at least three perfect numbers greater than 17.

Both of these sentences seem to have the form:

(3) There are at least three FG's that bear relation R to a.

The understanding of (2) on the model of (3) is dubbed *the standard view*. The standard view interprets quantification in mathematics as equivalent to quantification in other areas of study. Benacerraf contrasts the standard view with what he calls *combinatorial* views. The standard view takes the surface syntax of mathematical propositions seriously—understands them to involve names and quantification—and uses a Tarskian semantics to give their truth conditions. A combinatorial view, on the other hand, dispenses with a semantic account of truth conditions and equates truth with provability in some system. The combinatorial view has the advantage of conforming to the second of Benacerraf's concerns: it makes it clear how it is that mathematical propositions are known. No notion of abstract objects or our knowledge thereof is necessary for a combinatorial account of mathematical truth.[2]

The combinatorial view satisfies the second of Benacerraf's concerns at the expense of satisfying the first. Benacerraf also holds that the standard view satisfies the first concern

[2] In fact, Benacerraf is less strict than one might assume given the above description. It is important to note that, in discussing the combinatorial view, Benacerraf explicitly considers not only purely formal systems but also formal systems augmented with an ω-rule. There is no evidence in the text to suggest that having something other than a purely formal definition of mathematical truth poses special epistemological problems. This is important given that the account of mathematical truth that I will defend involves informal reasoning.

at the expense of the second. As is well known, Benacerraf's (1973) defense of this claim involves the causal theory of both knowledge and reference.

> I favor a causal account of knowledge on which for *X* to know that *S* is true requires some causal relationship between *X* and the referents of the names, predicates and quantifiers in *S*. I believe in addition in a causal theory of *reference*, thus making the link to my knowing *S doubly* causal. (p. 671)

Once one accepts a causal theory of knowledge and reference, platonism is ruled out. Platonists believe that mathematical objects are abstract, and thus not the kind of objects that we interact with in the physical world, but the standard view need not be platonistic. We could continue to hold that mathematical statements involve names and quantification, if mathematical objects were interpreted as objects in the physical world (i.e., objects we causally interact with). The naturalness of the standard view, however, would be lost. So, given a causal theory of knowledge and reference, the prospects for the standard view look bleak. For all that, whereas causal theories of truth and reference were the way to go in 1973, they no longer hold such appeal.

Hartry Field has offered a formulation of Benacerraf's dilemma that does not depend on a causal theory of knowledge (or reference). Most contemporary commentators therefore address Field's formulation instead of Benacerraf's. Field's idea is to change the focus from the *justification* of our mathematical beliefs to an *explanation* of their reliability. By doing so, Field (1989) avoids an appeal to any specific theory of knowledge or reference.

> [T]he claim [that the platonist must explain] is just that the following schema
>
> If mathematicians accept 'p', then p
>
> (and a partial but hard to state converse of it) holds in nearly all instances, where 'p' is replaced by a mathematical sentence (p. 26).

That this is essentially the Benacerraf problem should be clear. Disquotation preserves syntactic form, and so the mathematical statements under consideration still concern abstract objects as their surface syntax suggests. But rather than formulate the problem in terms of knowledge, which would therefore depend on one's epistemic theory, Field reformulates the problem in terms of reliability, which is independent of one's epistemic theory.[3]

Field does not claim that we can make no progress toward explaining the reliability of our mathematical beliefs. We could appeal to provability, but that will get us only so far.

> I do not mean to suggest that the platonist can do nothing towards explaining the general regularity given by [the disquotational schema] and its partial converse. For as mathematics has become more deductively systematized, the truth of mathematics has become reduced to the truth of a smaller and smaller set of basic axioms; so we could explain the fact that the mathematicians' beliefs tend to be true by the fact that we could

[3] There is one difference between Field's formulation and Benacerraf's besides the change of focus from justification to explanation: Field poses his version as a challenge to the defender of platonism whereas Benacerraf presents two apparently conflicting concerns.

> deduce them from the axioms, if we could just explain the fact that what mathematicians take as axioms tend to be true. (Field, 1989, p. 231)

Furthermore, Field allows that we can make some progress toward explaining the regular truth of the axioms by pointing out that the totality of what mathematicians take as axioms tends to be consistent. And we can in turn partially explain this by pointing out that over time inconsistencies with proposed axioms have been eliminated when actually discovered. However, Field argues that these partial explanations do not go nearly far enough. It would be absurd to explain the reliability of someone's beliefs about the physical world by pointing out that they follow from a set of axioms that have not yet been found to be inconsistent.

§3. Informal mathematics. For an account of the reliability of mathematicians' beliefs about the axioms of mathematics, we now turn to Kreisel's account of informal rigour. In his 1965 presentation at the *International Colloquium in the Philosophy of Science* Georg Kreisel defends a very commonsense view of mathematics. His presentation is published in 1967 as *Informal rigour and completeness proofs*. Although he stresses the importance of formalization in mathematics, Kreisel (1967) contends that mathematical reasoning begins by an analysis of our intuitive notions.

> The 'old fashioned idea' is that one obtains rules and definitions by analyzing intuitive notions and putting down their properties. This is certainly what mathematicians thought they were doing when defining length or area or, for that matter, logicians when defining rules of inference or axioms (properties) of mathematical structures such as the continuum. (p. 138)

Kreisel calls this process of examining our intuitive notions and determining their properties *informal rigour*.

Kreisel aims to criticize what he calls *pragmatist* or *positivistic* philosophies of mathematics. These 'anti-philosophic' philosophies of mathematics dismiss intuitive notions as being too vague to play any serious role in mathematics. Mathematics, according to such views, proceeds by stipulating arbitrary axioms and then deriving their formal consequences. Pragmatism rejects the importance of intuitive notions by laying emphasis on *fruitfulness* as opposed to *faithfulness*. Positivism, on the other hand, contends that the content of our intuitive notions lies exclusively in the formal operations that we perform with them.[4] What is wrong with these two views? "Quite simply this. *Though they raise perfectly legitimate doubts or possibilities, they just do not respect the facts, at least the facts of actual intellectual experience.*" (Kreisel, 1967, p. 141, original italics)

But beyond criticizing these 'anti-philosophic' philosophies, Kreisel's main aim is to demonstrate that we do have a sufficiently clear understanding of, for instance, the natural numbers or the *cumulative type structure* (the universe of sets). This understanding goes beyond what is captured by first-order formalizations. "A moment's reflection shows that the evidence for the first-order axiom schema derives from the second-order [axioms]." (Kreisel, 1967, p. 148)

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[4] It is unclear who falls under Kreisel's categories of 'pragmatist' or 'positivist'. Although, the category of positivist was intended to include Carnap, I have argued (Lavers, 2008) that this is not the case.

It may seem as if the paradoxes in set theory show that informal rigour is not actually very reliable. We apparently had an idea of set that seemed to conform to the naïve comprehension principle. Once we began to explore the properties of such sets we found that our intuitive concept is inconsistent. On Kreisel's diagnosis, however, it is not that we had an intuitive idea of an inconsistent concept. What we actually had was a vague concept that was a mixture of three intuitive notions: sets of individuals, sets of something (which satisfies only a restricted comprehension axiom), and properties or intensions. It is by examining our notion of set of something that we arrive at our understanding of the cumulative type structure.

> Zermelo's analysis furnishes an instance of a rigorous *discovery of axioms* (for the notion of set). To avoid trivial misunderstanding note this: What one means here is that the intuitive notion of the cumulative type structure provides a coherent *source* of axioms; our understanding is sufficient to avoid an endless string of ambiguities to be resolved by further basic distinctions, like the distinction above between abstract properties and sets *of* something. [...] the actual *formulation* of the axioms played an auxiliary rather than basic role in Zermelo's work: the intuitive analysis of the crude mixture of notions, namely the description of the type structure, led to the axioms: these constitute the record, not the instrument of clarification. (Kreisel, 1967, pp. 144–145, original italics)

Of course, Kreisel is aware of disputes within mathematics itself. The existence of these disputes might lead one to conclude that where our ordinary understanding of a notion gives no clear pronouncement, we are free to make arbitrary stipulations. Kreisel (1967) believes that this conclusion is unwarranted:

> [O]ne sometimes criticizes complacently 'old fashioned' disputes on the right definition of measure or the right topology, because there are several definitions. The most striking fact here is how *few* definitions seem to be useful: these haven't dropped from heaven; they, obviously, were formulated *before* their applications were made, and they were not generally obtained by trial and error. If they had been so obtained, mathematicians shouldn't be as contemptuous as they are about the study of little variants in definitions. (p. 142, original italics)

Kreisel's position then is that our ordinary understanding of the concepts *natural number* or *set*, for instance, played a central role in the development of number theory and set theory. The axioms are arrived at by an analysis of these notions. If something is independent of a certain axiom system we are not free to claim that it has no truth value (or to decide the matter by arbitrary stipulation), for a further analysis of our informal concepts may be sufficient to answer the question of its truth or falsity.[5] This is clearly a very natural

[5] Interestingly Field (1994, 1998) himself, in a series of papers, has argued that we could not possess a mathematical concept that could not be formalized in a first-order theory, and as a result our mathematical notions must be highly indeterminate. If considerations of space were not an issue I would argue that the situation is a stalemate between Field and the philosopher who wishes to defend the determinacy of mathematical notions. Field's (1994) position relies heavily on his assumption that "on any reasonable way of making 'implicitly accepts' precise, the set of sentences and inference rules that a person implicitly accepts at a given time is almost certainly

position on mathematical truth. What the rest of this paper will explore is the degree to which an acceptable answer to Benacerraf's dilemma can be built around this position.

§4. Epistemology and informal mathematics. As we saw above, Field demands of a supporter of the standard view an account of the reliability of our mathematical beliefs. In presenting his interpretation of the Benacerraf dilemma, Field (1989) writes:

> Benacerraf's challenge—or at least, the challenge which his paper suggests to me—is to provide an account of the mechanism that explains how our beliefs about these remote entities can so well respect the facts about them. The idea is, *if it appears in principle impossible to explain this*, then this tends to *undermine* the belief in mathematical entities, *despite* whatever reason we might have for believing in them. (p. 26, original italics)

We also saw above that Field accepts that the platonist can reduce the problem of explaining the reliability of mathematical propositions generally, to the problem of explaining the reliability of the axioms that we accept. Kreisel, on the other hand, defends a commonsense view on how we come to accept axioms. We accept axioms because we deem them to be true of our informal notions.

Now, the position Kreisel puts forward is not an explanation of how informal mathematics works. Kreisel is concerned to argue that we *can* do this, even if we don't yet know exactly *how*. In fact, he is explicit on this point in his joint work with Jean-Louis Krivine:

> We do not pretend to have a theory of a mechanism which explains how we come to form intuitive notions which are so astonishingly successful; for example how the founders of dynamics set up equations of motions for rigid bodies or ideal fluids on the basis of qualitative impressions and derived quantitative conclusions from these equations. But we regard it as absurd to reject the use of this ability just because we don't have a theoretical explanation; this is what the formalist doctrine of precision does. (Kreisel & Krivine, 1971, p. 169)

What should stand out about the previous two quotations is that both Kreisel and Field appear to be speaking about the same mechanism, that is, whatever mechanism is the one which allows us to write down axioms that are true of, for instance, sets or natural numbers. Field maintains that if it is in principle impossible to explain such a mechanism, we should conclude that we have no such ability. Kreisel's argument, of course, works the other way. Given that it is a fact of 'actual intellectual experience' that we have such an ability, it makes no sense to deny it just because we don't have a proper theoretical explanation of it. So the situation seems to be a classic case of one philosopher's modus ponens being another's modus tollens.

At this point it is important to make a distinction. The question of whether we have the ability to do informal mathematics in essentially the manner described by Kreisel can be interpreted in two ways. On the strong reading we analyze our intuitive notions and thereby obtain truths which hold in the completely independent and self-subsisting platonic realm of mathematical objects. However, there is a weaker reading that makes no

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recursive (and might well be finite)." (p. 402) It is unclear why the defender of the determinacy of our mathematical notions would have to accept this assumption.

such metaphysical claims. On the weaker reading we have an understanding of concepts like *set* and *natural number* that possess more properties than they can be shown to have in any particular formal system. Our understanding of these concepts can thus be a source of axioms. That is, prior to a formulation of an axiom system, there is at least a community that shares an informal notion.[6] Axioms are produced by analyzing this notion and identifying what is true of it. On this weak reading our ability to do informal mathematics does not commit us to any particular metaphysical view.

With this distinction in place we can see that Kreisel and Field do not actually—contrary to what seemed to be the case above—hold opposing positions on the same issue. Kreisel's arguments support what I have called the weak reading, while Field's discussion of the Benacerraf problem attacks the strong reading. Field (1989) believes the insurmountable problem for the platonist is to explain how we have knowledge of completely independently existing realms.

> It is rather as if someone claimed that his or her belief states about the daily happenings in a remote village in Nepal were nearly all disquotationally true, despite the absence of any mechanism to explain the correlation between those belief states and the happenings in the village. Surely we should accept this only as a last resort. (p. 27)

In order to avoid the problem illustrated in this quotation (which I will call *Field's Nepalese village objection*), I want to assume that we have the ability to do informal mathematics without making any assumption that this ability gives us access to completely independent truths. That is, I want to assume that we can do informal mathematics, roughly as Kreisel describes, but without any assumption that we thereby obtain truths concerning an independently existing domain. I will follow Kreisel and Field in taking this ability to be not fully explained. I will not offer an explanation of this ability, but merely assume, for the time being, that we possess it. So even with this assumption a complete answer to Field's challenge will not be given. A complete answer would have to include an explanation of this ability.

Saying that the ability to do informal mathematics is not fully explained is not to say that we have no idea how informal mathematics functions. Analyzing our intuitive notions to arrive at further axioms may seem like a mysterious process, but it has of course many straightforward instances. For instance consider the natural numbers. Let T be a first-order theory of arithmetic in a language L. Let L^+ extend L by the addition of new constants. Models of T for which induction does not hold for properties involving the new constants are not candidates for the structure we intend when we speak informally of the natural numbers. Instances of the induction schema involving the new properties could then be added as further axioms.

The idea that we are able to do informal mathematics amounts to holding that we do not formulate axioms on the basis of trial and error, but that there is an intuitive notion being analyzed prior to the formulation of the axioms, and relative to which the axioms are true. For instance, it holds of our conception of natural number that every natural number has a successor, and that induction holds for every property of the natural numbers. Likewise, by careful examination of our notion of 'set' we notice that the various axioms of set theory

[6] I do not want to rule out the limiting case where the community consists of only one person. The important point is that the informal notion exists prior to the formulation of the axioms, and that it *could be shared by more than one person.*

hold.[7] Notice, it is not being claimed that these informal notions are innate ideas or that they are shared universally by all human beings. All that is being claimed is that there is at least some group of mathematicians who understand the informal notion well enough to realize that the axioms are true of it. To deny this is to claim that mathematics is the study of arbitrary axiom systems.

Kreisel is well known for claiming that, by analyzing our informal notions, we could in principle determine the truth value of any mathematical statement of interest to us. If we examine our intuitions carefully enough, we can determine, for instance, an answer to the continuum hypothesis. However, we needn't follow Kreisel on this point. One may wish to hold that we have an understanding of notions like the 'natural numbers' that transcend any purely formal axiomatization without claiming that all mathematical questions can be answered by examining our intuitions. I believe this is a point about which the best course of action is to suspend judgment. We should hold that an examination of our intuitive notions may lead to a further axiom that settles the continuum hypothesis. However, I do not see why we should hold dogmatically to the idea that in principle all interesting mathematical questions can be answered.

What I wish to do in the remainder of this section is to explore the consequence of taking Kreisel's 'old-fashioned idea' seriously. That is, I would like to explore what view of mathematical truth follows naturally from the assumption that our understanding of informal notions plays an essential role in mathematics. To this end I will introduce the predicate Dem. Dem is to cover anything that is demonstrable on the basis of informal rigour. Axioms identified by an analysis of informal notions are then to have the property Dem. Since for the present purposes, I am interested in classical mathematical truth anything deducible in classical logic from the identified axioms is to have the property as well. Also, as Kreisel stresses, certain facts about the integers, for instance, are only provable by embedding the integers into a larger system. Propositions obtained then, by embedding one system in a larger one, are to have the property Dem as well.

Dem, it was said, is to represent the property of being demonstrable on the basis of informal rigour. Of course, this is far from a formal definition, but anyone who holds that what cannot be given a formal definition is so unclear that it can serve no purpose is strongly advised to read Kreisel's article. In most cases, but not all, it will be quite unproblematic to decide if some proposition has the property Dem or not. I certainly do not claim that there will be universal agreement in all cases, just that the borderline cases are the exception and not the rule.

There is clearly an obvious connection between what we call true in mathematics and the property Dem. This connection can be summarized in the following principle (called DIT for *demonstrability implies truth*):

$$(DIT) \quad Dem(\mathbf{A}) \supset \text{True}(\mathbf{A}).$$

When Tarski wished to define a truth predicate he wanted it to apply to those sentences which we ordinarily call true. Since we are interested in the concept of truth as it is used in the special case of mathematics, we ought to, if at all possible, seek a concept that applies to those sentences that we ordinarily call true. There may be overriding epistemic or metaphysical reasons why we cannot have such a concept, but in the absence of such

[7] Obviously much more could be, and has been, said on this point, but the subject will not be discussed further here. For a discussion of these issues see for instance Boolos (1983), Parsons (1983), and Wang (1983).

reasons we should attempt to formulate a conception of mathematical truth that applies to those sentences that we consider to be true. Without such reasons, anyone who holds a revisionist position is being revisionist for revisionism's sake. Furthermore, the goal here is to defend a position that satisfies both horns of Benacerraf's dilemma; it would certainly be too strict a requirement on such a position that it rule out the possibility of all views that abandon one of the two horns. In the next section I will defend the position that there are no overriding epistemic or metaphysical reasons forcing us to assume a revisionist attitude toward truth in mathematics.

Before moving on, however, I would like to briefly discuss an objection to the principle DIT. Accepting DIT, it could be argued, amounts to embracing a form of conventionalism. After all, accepting DIT amounts to stipulating that what we can identify as being *true of* some informal notion is *true*. The first thing that can be said in response to this charge is that every account of mathematical truth will involve some stipulations. Secondly, in defense against the charge of conventionalism, this restriction on mathematical truth is about the most natural one available. So, it may be a stipulation, but it is by no means an *arbitrary* stipulation.

§5. Ontology and informal mathematics. Accepting DIT, it might be claimed, amounts to what Benacerraf called a combinatorial view. However, I do not wish to replace truth with demonstrability. What I wish to show is that a view on mathematical truth that is not revisionist can be shown to be metaphysically and epistemically defensible.

DIT links truth with what we recognize as true of our intuitive notions. And these in turn involve names and quantification. For instance when examining our notion of natural number we can recognize both that *zero* is a natural number and that *all* natural numbers have a successor. It is in this form that they are suggested by examining our notion of natural number. It may be possible to translate these sentences into sentences that do not use names or quantify over numbers, but if we did this we could not cite our understanding of the concept of natural number as justification of the translated sentence.[8] Since we are here interested in a (nonrevisionist) notion of mathematical truth based on what we can determine by analysis of our mathematical concepts, we should take the syntax of the statements so suggested at face value. So like DIT a nonrevisionist principle which states that we should take the surface syntax of mathematical propositions at face value should be accepted if there are no overriding philosophical problems caused by accepting such a principle. In addition to DIT, then, a nonrevisionist about mathematical truth would have to accept ONT (for ontology):

$$(ONT) \quad \mathrm{True}(\mathbf{A}) \equiv \mathbf{A}.$$

Clearly, if DIT and ONT can be defended together then both horns of Benacerraf's dilemma will be satisfied. DIT explains how we know certain mathematical truths—the ones that we can demonstrate; while ONT implies that we take their syntax at face value. Let us look for a second specifically at Field's formulation of the problem. Recall that Field maintained that if we could explain how the axioms of mathematical theories are disquotationally true, then the rest follows. Well, any axioms identified by analysis of our informal notions are true by DIT, and ONT guarantees disquotation.

[8] Or at least not without a great deal more explanation.

Of course, this solution to Benacerraf's dilemma presupposes DIT and ONT, which have not yet been argued for. All that I have argued so far is that DIT and ONT should be accepted if there are no overriding metaphysical or epistemic reasons not to accept them. What I want to argue now is that a view of mathematical truth that conforms to DIT and ONT can be defended that avoids any insuperable epistemic or metaphysical problems. So what are the prima facie metaphysical and epistemic reasons against holding both DIT and ONT as constraints on accounts of mathematical truth? Well, together they imply that there are mathematical objects and that we can know about them simply by examining our intuitions and making deductive inferences. How, then, are we any better off than the person in Field's Nepalese village objection? In order to show that we are actually better off in the case of mathematics than the case of the person who claims disquotationally true beliefs about the events in a remote village of Nepal, we need to begin by addressing the question of how we know that there are mathematical objects with the properties we take them to have. I believe the most progress in terms of answering this question is to be found in the work of Frege.[9]

In section 62 of *The Foundations of Arithmetic*, Frege famously asks "How, then, are numbers to be given to us, if we cannot have ideas or intuitions of them?"[10] In giving his answer Frege (1980) cites his context principle which advises one "never to ask for the meaning of a term in isolation, but only in the context of a proposition." (p. x) Given the context principle the question of our knowledge of the numbers becomes the question of how we can fix the truth conditions for statements involving number. This move on Frege's part has rightly been recognized as revolutionary (see, for instance, Dummett, 1991, and Hale & Wright, 2002). A large obstacle to the acceptance of abstract objects is that it is unclear how we could have a coherent practice of making claims about them. Frege eliminates this obstacle by changing the epistemic question of our access to such objects into the semantic question of fixing truth conditions. Once we fix the truth conditions for claims involving number, there remains no mystery of how we have a coherent practice of establishing claims about the numbers.

In the case of the present paper we can follow Frege's example as follows. We put forward DIT as a constraint on mathematical truth. That is, we say that any more detailed account of mathematical truth must satisfy DIT. This fixes the truth conditions sufficiently well that we can have a coherent practice of establishing claims about mathematical objects. Remember, I am not committed to the Kreiselian view that we could in principle answer any mathematical question of interest by employing the method of informal rigour. This leaves it open that the truth conditions for mathematical statements may not be completely determined by DIT. However, even if this is so, DIT is sufficient

9 I look to Frege to show that a view that conforms to DIT and ONT can avoid insuperable metaphysical and epistemic problems. Despite this reliance on Frege, there are significant differences between the view I am defending and that of the neo-Fregeans. The principal difference is that I place no special weight on abstraction principles. A view based on the idea of Kreisel's informal rigour agrees with Quine that deciding what is an axiom and what is a theorem is like deciding which points in Ohio are starting points. Getting into too much detail on the similarities and differences between the account presented here would take us too far off course. The few comparisons that will be made will be, for the most part, limited to footnotes. The neo-Fregeans address Benacerraf's problem most directly in Hale & Wright (2002).

10 Of course, Kreisel does talk of our *intuitive* understanding of concepts like 'set' or 'natural number', but nothing he says commits him to the view that intuition is some quasi-perceptual faculty.

to ground a coherent practice of establishing mathematical claims.[11] We then follow Frege in saying that once we have sufficiently well determined the truth conditions for statements involving a class of terms there is no further question of whether these things really exist and how it is that we know this. Once we fix the truth conditions we are free to take the terms introduced to refer. That is, given a Fregean view on abstract objects and DIT as a constraint on accounts of mathematical truth we are justified in holding ONT.

At this point one might likely object to the Fregean view on abstract objects itself. The objector would claim that either there are numbers and sets and so forth or there are not. That is, either the world is a model of our mathematical theories or it is not. The Fregean picture, the objection continues, is predicated on the incoherent view that reality is an *amorphous lump* awaiting to be *carved up* by our theories.[12]

It is true, the philosophical view that reality is an amorphous lump is often dismissed as not quite coherent. The intuition behind it—that the-world-as-it-is-in-itself is not divided into discrete domains of various logical types[13]—may seem attractive enough, but given that a further clarification of this intuition is not forthcoming the view is often rejected. However, I think this rejection is too hasty. Does this mean I am advocating the acceptance of a notoriously vague philosophical position? No—the proper way to understand the view that reality is an amorphous lump, is not as a positive theory but as the rejection of a vague philosophical position. Notice even in the brief description above, the position is stated in the negative. The reading of the amorphous lump view as a positive thesis I will call the strong reading, and the view that merely rejects a certain thesis I will call the weak reading. The philosophical position being rejected by the weak reading is that *somehow* the-world-as-it-is-in-itself is the kind of thing that could be a model of our theories and as such plays the role of ultimately determining the correctness of our semantic theories. This position, rejected by the weak reading, I will call the ultra-realist position. The strong reading makes a positive claim about the ultimate structure of the world. That is that the world is such that we can carve it up with our language.

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11 The position I defend in this paper is realist about abstract objects. This is what is needed to satisfy the first horn of Benacerraf's dilemma. It is not realist in the sense of being committed to a completely bivalent semantics (although it would be compatible with this). Dummett (1978) argues that realism/antirealism debates should be understood to question the existence of realist truth conditions, instead of the existence of objects. This is motivated by the fact that the intuitionist about arithmetic is committed to the existence of numbers just as the classical number theorist is. For the present purposes, however, we are concerned with the existence of objects. Dummett often suggests that there is something illegitimate or incoherent about classical mathematics. Classical mathematical truth is neither incoherent nor illegitimate, it is a very clear and extremely useful notion worthy of analysis. It may, however, not be fully determinate. By taking the closure, under classical consequence, of the axioms we identify, we might not fix the truth conditions of every mathematical statement. If we consider a statement A for which neither it nor its negation has the property Dem, then although DIT implies the truth of $A \vee \sim A$ it neither implies the truth of A nor of $\sim A$. In this way, everything we take to be a truth of classical mathematics (including, all instances, of the law of the excluded middle) turns out to have the property Dem. However, I don't see this (potential) indeterminacy as reason to claim that we are not dealing with a domain of objects. Even the discourse concerning observable objects and their properties is not completely determinate.

12 The term 'amorphous lump' is taken from Dummett (1981).

13 Wittgenstein, of course, held that everything is of the same logical type. But how many logical types there are is not really the problem. What seems hard to swallow is that the world-as-it-is-in-itself is the kind of thing that could be a model of our theories.

Notice that the coherence of the ultra-realist view is tied to the coherence of the strong reading of the amorphous lump view. Are numbers objects? Somehow, the ultra-realist view contends, the-world-as-it-is-in-itself will decide such questions. If this view could be made clear, then clarifying the strong reading of the view that reality is an amorphous lump would be no problem at all—it would amount to claiming that ultimately reality does not have the structure of a model. One cannot reject the view that reality is *not* divided into discrete domains of various types on the basis that the view is unclear, and then simply proceed on the assumption that it is so arranged. To do this would be to argue: $\sim P$ cannot be made sufficiently clear, and so we must reject it and assert P. Of course, *under a particular description* the world can either be or fail to be a model of our theories. However, the question of whether, independently of any description, the world is a model of our theories cannot make more sense when answered in the affirmative than in the negative. The weak reading of the amorphous lump view, can reject both the strong reading and the ultra-realist position as incoherent.

It is perfectly possible to hold a Fregean view on abstract objects while rejecting as incoherent the question of whether, ultimately, the world is a model of our theories.[14] We take a term to refer when truth conditions for statements involving it have been sufficiently clearly spelled out, but we don't take this as making any claim about the ultimate composition of reality. What Frege gives us is a way of analyzing reference that does not depend on our first being able to identify how reality itself is divided into discrete objects.

One of the motivations, as we saw above, for Benacerraf's claim that mathematical statements involve names and quantification, was the desire to treat mathematical claims as on par with other claims in the language. It might be objected that accepting the Fregean view on abstract objects amounts to abandoning this motivation. However, this is not so. It is not only terms for abstract objects that we explain by virtue of them being part of a coherent practice involving sufficiently well-defined truth conditions for sentences containing them. There are no terms whose reference we explain by pointing to a relation between the term and the thing-in-itself. We can take the singular term 'Jones's car' to refer because we have a coherent practice of speaking about people's cars. The reason I take it to refer is not because I am aware of a Jones's car as a thing-in-itself and an ultimate constituent of the world-as-it-is-in-itself. If to know of an entity that it exists we must know that it is an ultimate constituent of the world-as-it-is-in-itself, then all ontological questions become hopeless. It is perhaps for this reason that the existence of every kind of thing has actually been disputed.

Although the view I am defending maintains that we can refer to and have knowledge of abstract objects, it may be objected that it is not sufficiently platonistic since it abandons the ultra-realist view. However, claiming that to answer Benacerraf's dilemma we must show that the numbers exist in the sense of the ultra-realist view is too strong a requirement. Benacerraf's dilemma is meant to be a problem about *abstract objects*. There is no generally accepted demonstration of the existence, in the ultra-realist sense, *of any kind of object*. One of the main obstacles facing those who wish to hold that we can have knowledge of abstract objects is the ultra-realist view on ontology. Notice, however, as just argued, this is

[14] The neo-Fregeans in their discussion of the Benacerraf dilemma (Hale & Wright, 2002) assume a 'quietist' position with respect to the question of which of these two views is correct. They admit that there are problems for their view no matter which of the positions is true, but rest content to endorse neither one. I think this is clearly not strong enough. That is why I maintain the question of whether the world is ultimately a model of our theories is to be rejected as incoherent.

no less an obstacle for nonmathematical objects. Once the ultra-realist view is abandoned, there is no longer an answer as to whether ONT is *ultimately* true. So, the goal is not to argue for the ultimate truth of ONT, but to show that it forms part of a natural and sufficiently unproblematic way of understanding mathematics. The Fregean position on reference to abstract objects together with DIT and ONT form such a view.

It can now be explained why it is that we are in a better position in mathematics than the person in Field's Nepalese village objection. If we ask how we know a certain mathematical proposition, we can point to the axioms that were identified by examining our informal notion as fixing the truth condition for statements involving the term. The person in Field's Nepalese village objection is claiming to have knowledge of propositions containing terms for which truth conditions, for statements involving those terms, are governed by practices that have nothing to do with anything that is accessible to the person in question. Above I advocated, and not just in the special case of mathematical terms, the Fregean view that for a term to refer it is sufficient that truth conditions are spelled out for a sufficiently large class of sentences that involve it. 'Seven' and 'Pawin', then, refer because statements involving them have sufficiently well-defined truth conditions. 'Seven is prime' unlike 'Pawin got married today' can be seen to be true on the basis of examining our informal notions and seeing what follows from them. This is not surprising since the truth condition for the first of these statements, but certainly not the second, is given in terms of what we can recognize as true of our informal notions.

§6. Objections to the view. It might be objected that the view being defended is very unFregean on the objectivity of mathematics.[15] Frege clearly wanted to establish the objective truth of mathematics. It could be argued that this realistic strand in Frege would lead him to claim that numbers exist prior to our 'carving up' reality by laying down truth conditions for arithmetical statements. That is, one might claim that Frege must be interpreted as holding the ultra-realist position. There is a lot to be said about Frege's position on this issue. However, this is not the place to settle Frege's views on this question. What I will argue here is that what Frege does in section 62 of *Grundlagen* cannot be seen as even a partial answer to the question of how we can have knowledge of abstract objects unless the strong form of realism discussed above is rejected. If it is one thing for something to be an object and another thing to be the reference of a singular term for which statements involving it have been given sufficiently clear truth conditions, then Frege has not answered how we can have knowledge of abstract objects. For, in this case, after spelling out the truth conditions for statements involving numerical terms, Frege would be left with the question of whether the things whose existence is implied by these truth conditions (i.e., the numbers) exist and how it is that we could know that this is the case.

Another Frege-inspired objection to the view being defended here is that it is too psychologistic. Above (Section 4) I made a distinction between two ways of interpreting the possibility of informal rigour. On the strong interpretation we analyze our informal concepts and thereby obtain truths that hold completely independently of us. On the weak interpretation our intuitive notions are a source of axioms but no claim is made that these axioms are independently true. It is the weak reading that I am defending here. Does the

[15] Although the view being defended here takes inspiration from Frege, especially section 62 of *Grundlagen*, it is not being claimed that the final view is a Fregean view. For that matter it is not being claimed either that the final view is Kreiselian.

claim that mathematics rests ultimately on what we can identify as true of our informal notions make mathematics overly psychologistic? Well, it certainly does not follow from the view I am defending that mathematical objects are ideas in people's minds.[16] Frege himself often assumes that either something is objective *and completely independent of us*, or it is subjective and psychological. But there is certainly room for objective claims that are not completely independent of us. In fact, this can be illustrated by playing with one of Frege's own examples. The equator is a fairly arbitrary collection of space-time points. One might hold that the equator is an ultimate constituent of the universe, but even those who hold an ultra-realist position about certain kinds of entities might be dubious of this. Truth conditions, therefore, for the claim that I am now more than 200 km from the equator, are in part dependent on us. This claim expresses a relation between me and an object that does not have completely independent existence.[17] But it is nonetheless objectively true that I am more than 200 km from the equator. In order to sufficiently fix the meaning of statements involving the equator we need only put forward a method for determining the distance of any point on the globe to the equator. Relative to these truth conditions many claims about the equator will be objectively true. That is, the conditions for being more than 200 km from the equator, although put forward by us, are objectively realized. In the case of mathematical objects introduced by DIT and ONT, although their truth conditions depend on us for their identification, relative to what we identify as holding of our informal notions mathematical theorems are objectively true.

One might also charge that the Fregean view being appealed to here is in danger of being applied too liberally. That is, if we are free to introduce any objects we wish simply by fixing the truth conditions governing statement involving the new terms, then we could introduce all kinds of undesirable objects. At this point it may seem that we are forced into one of two possible positions. First, we might claim with Carnap (1950), that any arbitrary specifications of truth conditions, for sentences involving a class of terms, describes a framework, and that there are only pragmatic reasons to prefer one framework over another. On this view, although certain frameworks may contain various undesirable objects, for pragmatic reasons we prefer frameworks that do not. Secondly, we could claim, along with the neo-Fregeans, that there are not several different frameworks, but there are independently motivated restrictions that in the end allow us to introduce only the objects that we want. I think, however, there is a third (and intermediate) possibility suggested by the Kreiselian position that we started with. Recall Kreisel's remark quoted earlier about mathematicians being contemptuous of slight variants in definitions. On Kreisel's 'old-fashioned' view we are not free to introduce any kind of object we wish so long as our axioms are consistent. We begin by examining our intuitive notions and writing down what is true of them. So already we see that a Kreiselean position would be more restrictive than Carnap's. We are not free to make arbitrary stipulations, but we proceed by identifying what we already take to be true. But accepting such a position involves accepting some degree of relativism. Remember it was never claimed that these intuitive notions are universal or innate. What was claimed was that prior to an axiomatization there is at least a group of

[16] In fact, given that ideas are finite in number, this is obviously incompatible with the view I am defending.

[17] Of course, one could claim that although the equator does not exist as a thing-in-itself, the statement's objective truth depends on it being translatable into a sentence whose terms do refer to ultimate constituents of the world. If this is the true criterion of objective truth, however, then we would have to admit that we have no idea what kinds of things are objectively true.

mathematicians who have a coherent informal notion and can recognize that the axioms are true relative to this notion. In the introduction to Potter (2000) there is a discussion of a group of people whose notion of natural number does not exceed five. If we assume that the anthropological facts reported here are true then what we identify as holding of our notion of number does not hold for theirs. Given that our mathematical notions are neither innate nor universal, it seems we need to acknowledge that mathematical truths are truths relative to a conceptual scheme. This is a less relativistic position than Carnap's but more so than Wright and Hale.

I want to turn now to an objection which could be made concerning the way I presented ontological questions above. One might say I set up a false dilemma. We need not hold that either reality can somehow play the role of ultimate arbiter of our semantic theories or that there is no notion of correctness or incorrectness in semantics. There is an intermediate position known to us. We can say, following Quine, that it is reality *as best described by our scientific theories* that decides if our semantic theory gets things right. However, the problem with this Quinean move is that there is no straightforward sense in which science tells us what there is.[18] There is no one science that tells us about all that exists, and there are many things that exist (such as the socket wrench set in my desk drawer) of which no science says anything.

One might avoid this objection by admitting that science will not tell us what specific things exist, but science tells us what general *kinds of things* exist. The existence of socket wrenches as a type of thing may be implied by history or anthropology. So, in this case, science can nonetheless answer, for instance, whether numbers exist. Now, if mathematics were itself considered to be one of the sciences that got to answer what types of things exist, then there would be nothing wrong with the ontological implications of the account of mathematical truth. The account defended in this paper implies the existence of numbers, and there is a science—mathematics—that also asserts the existence of such objects.

Of course, most who are sympathetic to the Quinean move would also restrict 'science' to include only the empirical sciences. With such a restriction, we are justified in asserting the existence of mathematical objects only if their existence is presupposed by empirical science. However, this seems to get the dependence backward. That we can't do physics without mathematics tells us more about physics than it does about mathematics. Even if tomorrow a brilliant new nominalistic physical theory were presented, one with every possible virtue, it simply would not be the case that mathematical theorems, like the propositions of phlogiston theory, would be viewed as a mere curiosity in the history of thought. Mathematics would not become simply a part of now-discarded physical theories, because the truth of mathematical claims is independent of their role in physical theories.[19] We can coherently engage in the practice of speaking of sets and natural numbers whether or not they play any role in any empirical science.

§7. Conclusions. I hope to have outlined above a very natural answer to the Benacerraf dilemma. If we are able to do informal mathematics in the way that Kreisel thought we could, then we have an understanding of certain concepts such that they possess more properties than can be demonstrated to hold of them in any formal system. We can then use this understanding of these concepts to lay out the truth conditions for statements involving

[18] The argument presented here is inspired by an argument in Hacker (2006).
[19] See Isaacson (2004) and again Hacker (2006) for more on this point.

them. If we take from Frege the view that in order to explain how we have knowledge of abstract objects we need only lay out the appropriate truth conditions, then there remain no epistemic or metaphysical problems regarding our knowledge of mathematical truths. That is, no problem beyond how it is that we are able to do informal mathematics in the first place. This remaining problem does not itself seem insurmountable. In fact, in the case of natural numbers we seem to have a fairly good understanding of how this works.

The most likely objection to the account given involves our freedom to stipulate that what we can identify as holding of our mathematical notions is true. In order to legitimate this move it was argued that our semantic theories are not responsible to the-world-as-it-is-in-itself, nor is there a clear sense in which science gives us an answer as to what there really is. Without any clear sense in which a theory of mathematical truth ultimately gets things right or wrong, it seems our only guide in selecting semantic theories is naturalness and agreement with common use. In these respects the theory under consideration does very well.

If then, one is willing to accept two things, a completely satisfactory answer to the Benacerraf problem can be given. The first is the assumption that our informal understanding of mathematical notions is sufficient to fix the truth conditions for statements involving mathematical terms. The second is the view that there is nothing else it is to be an object than to be the reference of a term for which sentences containing the term have been given sufficiently determinate truth conditions. This, then, at once solves the epistemic and ontological horns of Benacerraf's dilemma. Mathematical claims involve objects, since they contain terms and have sufficiently clear truth conditions, at the same time we can know them since we are able to do informal mathematics in the manner described.

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BIBLIOGRAPHY

Benacerraf, P. (1973). Mathematical truth. *Journal of Philosophy*, **70**(19), 661–679.

Boolos, G. (1983). The iterative conception of set. In Benacerraf, P., and Putnam, H., editors. *Philosophy of Mathematics* (second edition). Cambridge, UK: Cambridge University Press, pp. 486–502. Originally published in the *Journal of Philosophy*, **68**(1971), 215–232.

Carnap, R. (1950). Empiricism, semantics and ontology. *Revue International de Philosophie*, **4**, 20–40. Reprinted in *Meaning and Necessity: A Study in Semantics and Modal Logic* (second edition). Chicago, IL: University of Chicago Press, 1956.

Dummett, M. (1978). Realism. In *Truth and Other Enigmas*. Cambridge, MA: Harvard University Press, 145–165.

Dummett, M. (1981). *Frege: Philosophy of Language* (second edition). London: Duckworth.

Dummett, M. (1991). *Frege: Philosophy of Mathematics*. Cambridge, MA: Harvard University Press.

Field, H. (1989). *Realism, Mathematics & Modality*. New York, NY: Basil Blackwell Ltd.

Field, H. (1994). Are our logical notions highly indeterminate? In French, P. A., Uehling, T. E., and Wettstein, H. K., editors. *Midwest Studies in Philosophy*, *Philosophical Naturalism,* Vol. XIX. Notre Dame, IN: University of Notre Dame Press, pp. 391–429.

Field, H. (1998). Do we have a determinate conception of finiteness and natural number? In Schirn, M., editor. *The Philosophy of Mathematics Today*. New York, NY: Oxford, pp. 99–130.

Frege, G. (1980). *The Foundations of Arithmetic* (second revised edition). Evanston, IL: Northwestern University Press.

Hacker, P. (2006). Passing by the naturalistic turn: On quine's cul-de-sac. *Philosophy*, **81**(2), 231–253.

Hale, B., & Wright, C. (2002). Benacerraf's dilemma revisited. *European Journal of Philosophy*, **10**(1), 101–129.

Isaacson, D. (2004). Quine and logical positivism. In Gibson, R. F., editor. *The Cambridge Companion to Quine*. New York, NY: Cambridge University Press, pp. 214–269.

Kreisel, G. (1967). Informal rigour and completeness proofs. In Lakatos, I., editor. *Problems in the Philosophy of Mathematics*. New York, NY: Humanities Press, pp. 138–186.

Kreisel, G., & Krivine, J.-L. (1971). *Element of Mathematical Logic*. Amsterdam, The Netherlands: North-Holland.

Lavers, G. (2008). Carnap, formalism, and informal rigour. *Philosophia Mathematica*, **16**(1), 4–24.

Parsons, C. (1983). What is the iterative conception of set? In Benacerraf, P., and Putnam, H., editors. *Philosophy of Mathematics*. Cambridge, UK: Cambridge University Press, second edition, pp. 503–529. Originally published in the *Proceedings of the 5th International Congress on Logic, Methodology and Philosophy of Science 1975, Part I: Logic , Foundations of Mathematics and Computability Theory*. Dordrecht, the Netherlands. Butts, R., and Hintikka, J., editors. Reidel, D. (1977).

Potter, M. (2000). *Reason's Nearest Kin*. Oxford: Oxford University Press.

Wang, H. (1983). The concept of set. In Benacerraf, P., and Putnam, H., editors. *Philosophy of Mathematics*. Cambridge, UK: Cambridge University Press, second edition, pp. 530–570. Originally published in H. Wang *From Mathematics to Philosophy*. London: Routledge and Kegan Paul 1974, pp. 181–223.

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